Bandolier 80

What do we think?
What do we know?
What can we prove?

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BIAS

One of ***Bandolier***'s little pleasures is to travel around the UK talking to groups of people in the NHS about EBM and the power of systematic literature appraisal. Over the past five years attitudes have changed. The initial (probably appropriate) apprehension or even (inappropriate) outright hostility has been replaced by a grudging acceptance coupled with a concern about the quality of systematic reviews and guidance based on them.

Bandolier thinks that concern is justified. Concern occasionally arises because the question asked by a systematic review is not the one that professionals want answered. For instance, reviews of antibiotic use for otitis media may say that use of antibiotics makes no difference at a week, but the question ***Bandolier*** is asked is whether it makes any difference over 24 to 48 hours? We could find no reviews that answered this.

The major concern, though, comes from the complete lack of understanding by readers of reviews, users of reviews, and most frighteningly by producers of reviews of the enormous potential of bias in studies to produce the wrong answer. Bias (almost) always works to increase the apparent effect of a treatment and show it to be better than it is. It is pervasive, and its effects are large. So this month ***Bandolier*** has produced a survivor's guide to bias.

Electronic update

Bandolier continues to build its Internet site. This month many more abstracts of evidence have been added to the alternative therapies, healthy living, pain, migraine and management sites. Many thanks to those who have asked to use ***Bandolier***'s 10-tips for healthy living (***Bandolier*** 78), including medical journalists. We have added a BMI chart on the Internet site and are actively searching for sponsors to enable us to build this area more quickly.

If you want to use anything from ***Bandolier***, feel free. All we ask is that you mention ***Bandolier*** and include the Internet address (www.ebandolier.com).

Paper update

We continue to work to ensure the future of ***Bandolier*** on paper for the many people who do not have fast Internet access at work. Stories have flooded in about how difficult some of you find this. The one we especially liked was the hospital with a single computer with Internet access, available in a special room in the library, and available only a few hours a week.

Bandolier has been struck of late, "many a time and oft'", by the continuing and cavalier attitude towards bias in clinical trials. We know that the way that clinical trials are designed and conducted can influence their results. Yet people still ignore known sources of bias when making decisions about treatments at all levels.

What is bias?

A dictionary definition of bias is "a one-sided inclination of the mind". In our business it defines a systematic disposition of certain trial designs to produce results consistently better or worse than other trial designs.

Garbage in, garbage out

For the avoidance of doubt, the clinical bottom line is that wherever bias is found it results in a large over-estimation of the effect of treatments. Poor trial design makes treatments look better than they really are. It can even make them look as if they work when actually they do not work.

This is why good guides to systematic review suggest strategies for bias minimisation by avoiding including trials with known sources of bias. They further suggest performing sensitivity analysis to see whether different trial designs are affecting results in a systematic review.

But this advice is ignored more often than not. It is ignored in reviews, and it is ignored in decision-making. The result is that decisions are being made on incorrect information, and they will be wrong.

Bandolier bias guide

Bandolier has therefore decided to revisit some of the words written on bias in these pages and elsewhere, and collect

In this issue

*The views expressed in **Bandolier** are those of the authors, and are not necessarily those of the NHSE*

them into one handy reference guide. The guide can be used when examining a systematic review, or a single clinical trial. The guide is not to be used for observational studies, or for studies of diagnostic tests.

Randomisation

The process of randomisation is important in eliminating selection bias in trials. If the selection is done by a computer, or even the toss of a coin, then any conscious or subconscious attitude of the researcher is avoided.

Some of the most influential people in evidence-based thinking showed how inadequate design exaggerated the effect measured in a trial (Table). They compared trials in which the authors reported adequately concealed treatment allocations with those in which randomisation was either inadequate or unclearly described, as well as examining the effects of exclusions and binding.

The results were striking and sobering, as the Table shows. Odds ratios were exaggerated by 41% in trials where treatment allocation was inadequately concealed, and by 30% when the process of allocation concealment was not clearly described.

Many systematic reviews exclude non-randomised trials because of the amount of bias arising from failure to randomise. ***Bandolier*** believes that restricting systematic reviews to include only randomised studies makes sense for reviews of treatment efficacy. The reason is the many, many examples where non-randomised studies have led reviews to come to the wrong conclusion.

Examples abound. A classic example (***Bandolier*** 37) is a review of transcutaneous nerve stimulation (TENS) for post-operative pain relief (Figure 1). Randomised studies overwhelmingly showed no benefit over placebo, while non-randomised studies did show benefit. Particularly where a review counts votes (a study is positive or negative) rather than combines data in a meta-analysis the randomisation effect is strong. It applies particularly to studies in alternative therapies.

Figure 1: Effect of randomisation on outcome of trials of TENS in acute pain

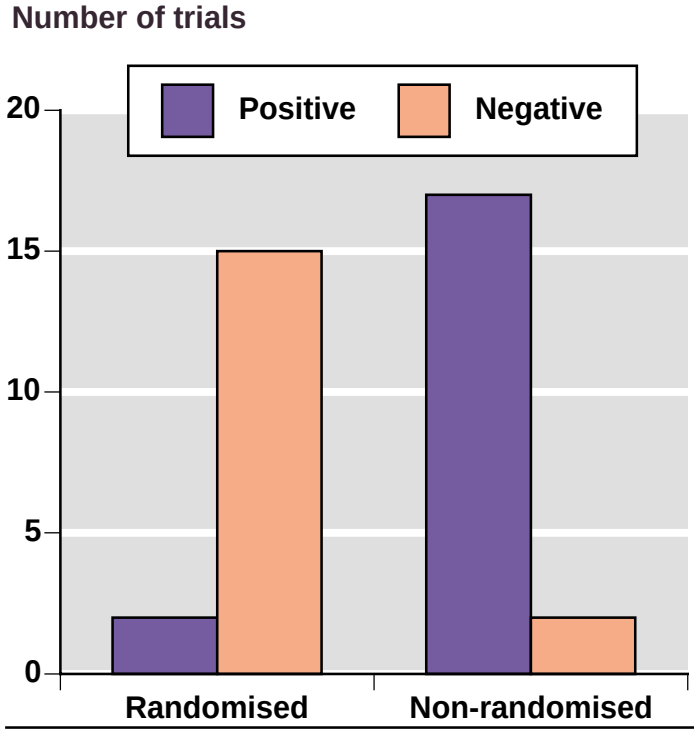
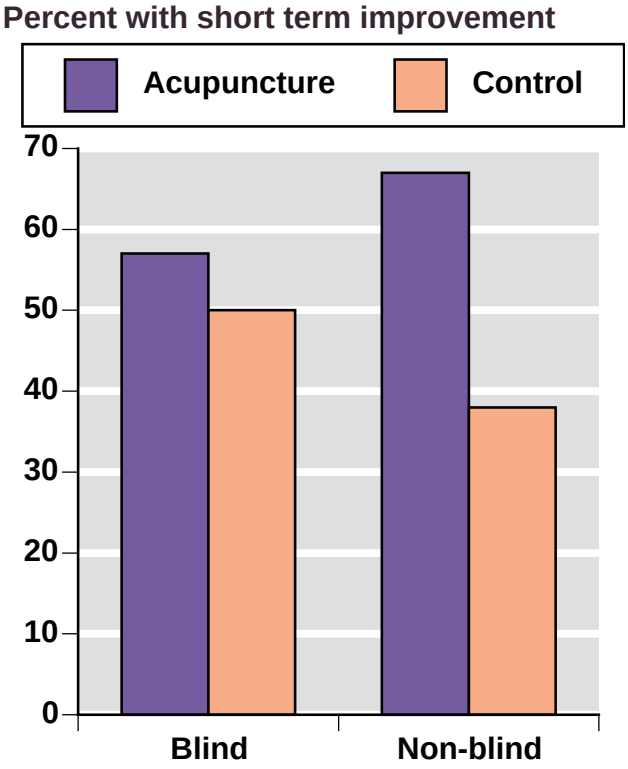


Figure 2: Effect of blinding on outcome of trials of acupuncture for chronic back pain



Blinding

The importance of blinding is that it avoids observer bias. If no-one knows which treatment a patient has received, then no systematic over-estimation of the effect of any particular treatment is possible.

Non-blinded studies over-estimate treatment effects by about 17% (Table). In a review of acupuncture for back pain (Figure 2), including both blinded and non-blinded studies changed the overall conclusion (***Bandolier*** 60). The blinded studies showed 57% of patients improved with acupuncture and 50% with control, a relative benefit of 1.2 (95% confidence interval 0.9 to 1.5). Five non-blinded studies showed a difference from control, with 67% improved with acupuncture and 38% with control. Here the relative benefit was significant at 1.8 (1.3 to 2.4).

Reporting quality

Because of the large bias expected from studies which are not randomised or not blind, a scoring system [1] that is highly dependent on randomisation and blinding will also correlate with bias. Trials of poor reporting quality consistently over estimate the effect of treatment (Table). This particular scoring system has a range of 0 to 5 based on randomisation, blinding and withdrawals and dropouts. Studies scoring 2 or less consistently show greater effects of treatment than those scoring 3 or more.

Table: Examples of known bias in trials of treatment efficacy

Source of bias	Effect on treatment efficacy	Size of the effect	References
Randomisation	Increase	Non-randomised studies overestimate treatment effect by 41% with inadequate method, 30% with unclear method	KF Schultz, I Chalmers, RJ Hayes, DG Altman. Empirical evidence of bias: Dimensions of methodological quality associated with estimates of treatment effects in controlled trials. Journal of the American Medical Association 1995 273: 408-12.
Randomisation	Increase	Completely different result between randomised and non-randomised studies	Carroll D, Tramèr M, McQuay H, Nye B, Moore A. Randomization is important in studies with pain outcomes: systematic review of transcutaneous electrical nerve stimulation in acute postoperative pain. British Journal of Anaesthesia 1996; 77: 798-803.
Blinding	Increase	17%	KF Schultz, I Chalmers, RJ Hayes, DG Altman. Empirical evidence of bias: Dimensions of methodological quality associated with estimates of treatment effects in controlled trials. Journal of the American Medical Association 1995 273: 408-12.
Blinding	Increase	Completely different result between blind and non-blind studies	Ernst E, White AR. Acupuncture for back pain: A meta-analysis of randomised controlled trials. Arch Int Med 1998, 158: 2235-2241.
Reporting quality	Increase	About 25%	Khan KS, Daya S, Jadad AR. The importance of quality of primary studies in producing unbiased systematic reviews. Arch Intern Med 1996,156 :661-6. Moher D, Pham B, Jones A, et al. Does quality of reports of randomised trials affect estimates of interventi
Duplication	Increase	About 20%	Tramèr M, Reynolds DJM, Moore RA, McQuay HJ. Effect of covert duplicate publication on meta-analysis; a case study. BMJ 1997, 315: 635-40.
Geography	Increase	May be large for some alternative therapies	Vickers A, Goyal N, Harland R, Rees R. Do certain countries produce only positive results? A systematic review of controlled trials. Control Clin Trial 1998, 19: 159-166.
Size	Increase	Small trials may overestimate treatment effects by about 30%	Moore RA, Carroll D, Wiffen PJ, Tramèr M, McQuay HJ. Quantitative systematic review of topically-applied non-steroidal anti-inflammatory drugs. BMJ 1998, 316: 333-8. Moore RA, Gavaghan D, Tramèr MR, Collins SL, McQuay HJ. Size is everything - large amounts of information are needed to overcome random effects in estimating direction and magnitude of treatment effects. Pain 1998, 78: 217-220.
Statistical	Increase	Not known to any extent, probably modest, but important especially where vote-counting occurs	Smith LA, Oldman AD, McQuay HJ, Moore RA. Teasing apart quality and validity in systematic reviews: an example from acupuncture trials in chronic neck and back pain. Pain 2000, 86: 119-132.
Validity	Increase	Not known to any extent, probably modest, but important especially where vote-counting occurs	Smith LA, Oldman AD, McQuay HJ, Moore RA. Teasing apart quality and validity in systematic reviews: an example from acupuncture trials in chronic neck and back pain. Pain 2000, 86: 119-132.
Language	Increase	Not known to any extent, but may be modest	Egger M, Zellweger-Zähner T, Schneider M, Junker C, Lengeler C, Antes G. Language bias in randomised controlled trials published in English and German, Lancet 1997 350: 326-329.
Publication	Increase	Not known to any extent, probably modest, but important especially where there is little evidence	M Egger, G Davey Smith. Under the meta-scope: potentials and limitations of meta-analysis. In M Tramèr, Ed. Evidence Based Resource in Anaesthesia and Analgesia. BMJ Publications, 2000.

Duplication

Results from some trials are reported more than once. This may be entirely justified for a whole range of reasons. Examples might be a later follow up of the trial, or a re-analysis. Sometimes, though, information about patients in trials is reported more than once without that being obvious, or overt, or referenced. Only the more impressive information seems to be duplicated, sometimes in papers with completely different authors. A consequence of covert duplication would be to overestimate the effect of treatment (Table).

Geography

In ***Bandolier*** 71 we reported on how geography can be a source of bias in systematic reviews. Vickers and colleagues

(Table) showed that trials of acupuncture conducted in east Asia were universally positive, while those conducted in Australia/New Zealand, north America or western Europe were positive only about half the time. Randomised trials of therapies other than acupuncture conducted in China, Taiwan, Japan or Russia/USSR were also overwhelmingly positive, and much more so than in other parts of the world. This may be a result of an historical cultural difference, but it does mean that care should be exercised where there is a preponderance of studies from these cultures. Again, this is particularly important for alternative therapies.

Size

Clinical trials should have a power calculation performed at the design stage. This will estimate how many patients are needed so that, say, 90% of studies with X number of patients would show a difference of Y% between two treatments. When the value of Y is very large, the value of X can be small. More often the value of Y is modest, or small. In those circumstances, X needs to be larger, and more patients will be needed in trials for them to have a hope of showing a difference.

Yet clinical trials are often ridiculously small. ***Bandolier***'s record is a randomised study on three patients in a parallel group design. But when are trials so tiny that they can be ignored? Many folk take a pragmatic view that trials with fewer than 10 patients per treatment arm should be ignored, though others may disagree.

There are examples where sensitivity in meta-analysis has shown small trials to have a larger effect of treatment than smaller trials (Table). The degree of variability between trials of adequate power is still large, because trials are powered to detect that there is a *difference* between treatments, rather than *how big* that difference is.

Figure 3: Trials of ibuprofen in acute pain that are randomised, double blind, and with the same outcomes over the same time in patients with the same initial pain intensity

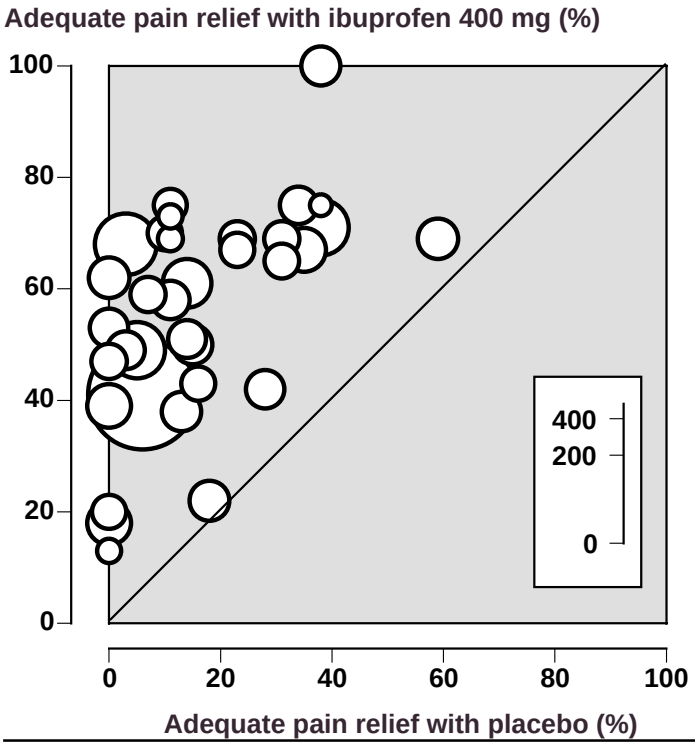
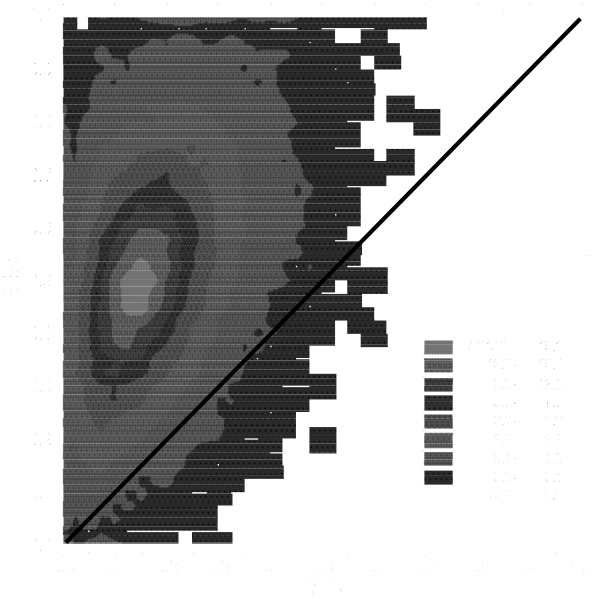


Figure 4: Computer model of trials of ibuprofen in acute pain. Intensity of colour matches probability of outcome of a single trial



In Figure 4 CER (control event rate) is equivalent to placebo and EER (experimental event rate) to ibuprofen in clinical trails

The random play of chance can remain a significant factor despite adequate power to detect a difference. Figure 3 shows the randomised, double blind studies comparing ibuprofen 400 mg with placebo in acute postoperative pain. The trials had the same patient population, with identical initial pain intensity and with identical outcomes measured in the same way for the same time using standard measuring techniques. There were big differences in the outcomes of individual studies.

Figure 4 shows the results of 10,000 studies in a computer model based on information from about 5,000 individual patients [2]. Anywhere in the gray area is where a study could occur just because of the random play of chance. And for those who may think that this reflects on pain as a subjective outcome, the same variability can be seen in other trial settings, with objective outcomes.

Statistics, data manipulation and outcomes

Despite the best efforts of editors and peer reviewers, some papers are published that are just plain wrong. Wrong covers a multitude of sins, but two are particularly important.

Statistical incorrectness can take a variety of guises. It may be as simple as the data presented in a paper as statistically significant not being significant. It can often take the form of inappropriate statistical tests. It can be data trawling, where a single statistical significance is obtained and a paper written round it. Reams could be written about this, but the simple warning is that readers or reviewers of pa-

pers have to be cautious of results of trials, especially where vote-counting is being done.

But also beware the power of words. Even when statistical testing shows no difference, it is common to see the results hailed as a success. While that may sound silly when written down, even the most cynical of readers can be fooled into drawing the wrong conclusion. Abstracts are notorious for misleading in this way.

Data manipulation is a bit more complicated to detect. An example would be an intervention where we are not told what the start condition of patients is, nor the end, but that at some time in between the rate of change was statistically significant by some test with which we are unfamiliar. This is done only to make positive that which is not positive, and the direction of the bias is obvious (Table). Again, crucially important where vote counting is being done to determine whether the intervention works or not.

Outcomes reported in trials are an even more sticky problem. It is not infrequent that surrogate measures are used rather than an outcome of real clinical importance. Unless these surrogate measures are known unequivocally to correlate with clinical outcomes of importance, then an unjust sense of effectiveness could be implied or assumed.

Validity

Do individual trials have a design (apart from issues like randomisation and blinding) that allows them to adequately measure an effect? What constitutes validity depends on the circumstances of a trial, but studies often lack validity. A validity scoring system applied to acupuncture for back and neck pain demonstrated that trials with lower validity were more likely to say that the treatment worked than those that were valid (Table).

Language

Too often the search strategy for a systematic review or meta-analysis restricts itself to the English language only. Authors whose language is not English may be more likely to publish positive findings in an English language journal, because these would have a greater international impact. Negative findings would be more likely to be published in non-English language journals (Table).

Publication

Finally there is the old chestnut of publication bias. This is usually thought to be the propensity for positive trials to be published and for negative trials not to be published. It must exist, and there is a huge literature about publication bias.

Bandolier has some reservations about the fuss that is made, though. Partly this stems from the failure to include assessments of trial validity and quality. Most peer reviewers would reject non-randomised studies, or those where there are major failings in methodology. These trials will be hard to publish. Much the same can be said for dissertations or theses. One attempt to include theses [3] found 17 dissertations for one treatment. Thirteen were excluded because of methodological problems, mainly lack of randomisation, three had been published and were already included in the relevant review, and one could be added. It made no difference.

Bandolier is also sceptical that funnel plots are in any way helpful. One often quoted, of magnesium in acute myocardial infarction [4], can more easily be explained by the fact that trials in a meta-analysis were trivially small to detect any effect and should never have been included in a meta-analysis in the first place.

But these are quibbles. If there is sufficient evidence available, large numbers of large, well conducted trials, then publication bias is not likely to be a problem. Where there is little information, small numbers of low quality trials, then it becomes more problematical.

Comment

This is but a brief review of some sources of bias in trials of treatment efficacy. Others choose to highlight different sources of potential bias. That bias is present, and exists in so many different forms is why we have to be vigilant when reading about a clinical trial, and especially when taking the results of a single trial into clinical practice.

But systematic reviews and meta-analyses also suffer from quality problems. They should consider potential sources of bias when they are being written. Many do not, and will therefore mislead. If systematic reviews or meta-analyses include poor trials or have poor reporting quality, then, just like individual trials, they too have a propensity a greater likelihood of a positive result [4,5].

There is no doubt that meta-analyses can mislead. If they do, then it is because they have been incorrectly assembled or incorrectly used. The defence, indeed the only defence, is for readers to have sufficient knowledge themselves to know when the review or paper they are reading should be confined to the bin.

References:

1 Jadad AR, Moore RA, Carroll D, Jenkinson C, Reynolds DJM, Gavaghan DJ, McQuay HJ. Assessing the quality of reports of randomized clinical trials: is blinding necessary? Control Clin Trial 1996, 17: 1-12.

2 Moore RA, Gavaghan D, Tramèr MR, Collins SL, McQuay HJ. Size is everything - large amounts of information are needed to overcome random effects in estimating direction and magnitude of treatment effects. Pain 1998, 78: 217-220.

3 A Vickers, C Smith. Incorporating data from dissertations in systematic reviews. Int J Technol Assess Health Care 2000 16:2: 711-713.

4 Jadad AR, McQuay HJ. Meta-analysis to evaluate analgesic interventions: a systematic qualitative review of the literature. J Clin Epidemiol 1996, 49:235-243.

5 Smith L, Oldman A. Acupuncture and dental pain. Br Dent J 1999, 186: 158-159.

ACUPUNCTURE TRIALS AND QUALITY

Acupuncture is commonly believed to be effective for the treatment of chronic pain, despite growing evidence from systematic reviews (***Bandolier*** 60) that it is not. Another systematic review across all of chronic pain draws attention to the fact that the number of clinical trials of high quality that showed acupuncture to be effective is risibly small. There are only three, at best.

Search

The search strategy was heroic, using standard databases and at least four specialist databases for complementary therapies as well as dissertation abstracts and conference proceedings. For inclusion trials had to be randomised, have a comparison group, have studied patients with pain longer than three months, used needles, were in English, and measured pain relief. Authors were contacted for details if necessary.

Outcomes

Trials were defined as positive if acupuncture was found to be significantly better than control, neutral if there was no significant difference between acupuncture and control, and negative when acupuncture was significantly worse than control. A P value of 0.05 was used to define statistical significance.

Results

There were 50 trials with 2,394 patients. Thirty-four trials (68%) had a quality score of 2 or less on a five point scale. Controls included waiting lists, inert controls, sham acupuncture and active controls, usually transcutaneous electrical nerve stimulation.

The main results are shown in the Table. Most high quality studies either showed no benefit or that acupuncture was worse than control. Forty to fifty percent of the trials or patients showed acupuncture to be better than control. Studies of low methodological quality showed significantly higher treatment effect than those of high quality.

Comment

Without labouring the point about poor quality studies overestimating effects of treatment, or that evidence for acupuncture is thin on the ground, this study demonstrates both with some clarity. It is worth commenting that for chronic pain where acupuncture is much used, the absence of significant effect is all too apparent. Only three studies showed a benefit, and they contained only 12% of the total patients studied in high quality trials. As a balance, one trial with 9% of the total patients studied in high quality trials showed less effect than an active treatment. The remainder of the high quality studies showed no difference at all between acupuncture and control.

What about bias? Well, the search was only for English language papers, but most non-English papers would be from countries where only positive results are published, so that this strategy may have avoided bias. Many of the studies were small. Of the 19 positive studies, 14 enrolled fewer than 50 patients, and the smallest number was 12. Overall, positive studies were smaller than neutral studies, which were smaller than negative studies. We might conclude that there is some residual bias in this, which would result in an even more negative conclusion.

There are two bottom lines. The first is to emphasise again the importance of using quality information for decision-making. Use poor quality information is likely to result in poor quality decision-making. The second bottom line is that the use of acupuncture for chronic pain is unsupported by any evidence of quality. Consumers and providers should beware.

References:

1 J Ezzo et al. Is acupuncture effective for the treatment of chronic pain? A systematic review. Pain 2000 86: 217-225.

Table: Effect of quality of trial reporting on whether trials of acupuncture in chronic pain are better, the same, or worse than control

	Number of trials	Percent	Number of patients	Percent
Quality score 3 or more				
Acupuncture better than control	3	19	111	12
Acupuncture same as control	12	75	715	79
Acupuncture worse than control	1	6	77	9
	16	100	903	100
Quality score 2 or less				
Acupuncture better than control	16	47	643	43
Acupuncture same as control	16	47	736	49
Acupuncture worse than control	2	6	112	8
	34	100	1491	100

SKULL X-RAY FOR MILD HEAD INJURY?

Head injury is not uncommon. For most (80-90%) people who sustain a head injury it is mild, and they need neither admission to hospital or complex health care. The worry is the development of intracranial haemorrhage. A meta-analysis [1] of radiological diagnosis indicates that a plain skull X-ray is of little value in initial assessment.

Mind you, after reading this necessarily complex paper, one feels as if a mild head injury would come as a welcome relief. That is not any adverse comment on the authors, who have done a fantastic job, but on the innate difficulty in sorting out diagnostic tests. This is another example of where the archaeology of past attempts to sort out the value of a test shows them to crumble to dust when exposed to the bright lights of contemporary reasoning.

Searching

A number of databases were used to find studies that were relevant on two counts. First were studies that informed about the prevalence of intracranial haemorrhage in patients with mild head injury. Second were studies that informed on the diagnostic value of a finding of skull fracture. Prospective and retrospective studies were included.

Mild head injury was defined as trauma to the head with a Glasgow coma score of 13 to 15 on initial presentation. The diagnosis of intracranial haemorrhage was sensible, ideally requiring a CT scan, though uneventful recovery in the absence of a CT scan was considered to indicate the absence of intracranial haemorrhage. Studies with fewer than 50 patients were excluded, as were those on children and older people.

Results

The mean prevalence of intracranial haemorrhage after mild head injury was 8% (95% confidence interval 3% to 13%) in 13 studies with 12,750 patients. Loss of consciousness or post traumatic amnesia occurred in 61% to 100% of patients in individual studies (most commonly 100%).

Differences in patient selection and the percentage of patients receiving a CT scan to verify skull fracture were important sources of variation between studies. This variation was large. Sensitivity varied between 13% and 75%, and specificity between 91% and 99.5%. The mean sensitivity was 50% at a specificity of 97%. For studies that had less bias where more than 50% of patients had loss of consciousness or post trauma amnesia and more than 50% had a CT scan, the mean sensitivity was 38% at a mean specificity of 95%.

What does this mean in practice? For a hypothetical 1000 patients with mild head injury, 83 would have an intracranial haemorrhage and 917 would not (Table). The likelihood ratio for a positive test for skull fracture was 7.7, raising the post-test probability to about 35%. The likelihood ratio for a negative test for skull fracture was 0.7, with a post-test probability of about 5%.

Comment

Like many, even most, reviews of diagnostic tests this one is hard to get to grips with. The simple take home message from the authors was that plain skull X-ray has no place in the assessment of mild head injury in adult patients. If an intracranial haemorrhage is not seen on a plain skull X-ray, then intracranial haemorrhage can still not be ruled out. We are reminded as well that it depends on who reads the X-ray: experienced physicians miss up to 10% of skull fractures. The authors of the review further concluded that patients with a Glasgow coma score of 15 with loss of consciousness or post trauma amnesia, and patients with a score of 13 or 14, require observation, a CT scan, or both.

This is useful information. The archaeology of the extant literature helps explain differences between studies of diagnostic tests or strategies. Even more it informs on how to design studies that might in future help better identify patients at risk of developing an intracranial haemorrhage after mild head injury. The lesson of this review is that when it comes to making effective and efficient diagnosis we need much better information than we have now. More archaeology won't help. We need new studies constructed to a better design, and providing outputs that will actually help in everyday situations. It may be expensive, and it may take time, but without better information we can't do better.

Table: Findings for a hypothetical population of 1000 patients with mild head injury

	Intracranial haemorrhage	
	Present	**Absent**
Fracture present	32	46
Fracture absent	51	871
Total	83	917

Sensitivity of positive test	0.39
Specificity of negative test test	0.95
Likelihiood ratio for a positive test	7.7
Likelihiood ratio for a negative test test	0.6

References:
1 PA Hofman, P Nelemans, GJ Kemerink, JT Wilmink. Value of radiological diagnosis of skull fracture in the management of mild head injury: meta-analysis. J Neurol Neurosurg Psychiatry 2000 68: 416-422.

Book reviews

Evidence-based medicine: how to practice and teach EBM. D Sackett, S Straus, WS Richardson, W Rosenberg, B Haynes. Churchill Livingstone pp 260 plus CD ROM. ISBN 0-443-06240-4.

This is the second edition of this marvellous book, one that has changed the lives of many of us. This second edition is more than the book, though, because it comes with a CD-ROM and associated website. Is it any good?

There is no question that for those of us who want either to do better ourselves, or to help others do better, this is THE book. In the 1960s there was a film adaptation of HG Well's "The Time Machine". In the last scene the hero returns and takes just three books off to the future to create a new society. This book would be one of the three.

It isn't just that the book walks us through ways of looking at diagnosis, treatment, prognosis, harm, guidelines, teaching and many other things besides. It's the way it does it: non threatening, non academic, not in any way superior. It is written by people who have been where you are now, who have felt the frustration or despair that you are feeling now, and who have found a way to help. Having this book is like having a friend at your elbow. Since ***Bandolier*** received a copy to review, it hasn't left our side.

What's changed since the first edition? Perhaps not so much the basic ideas, but the clarity with which they are conveyed. EBM is actually easy once you have some basic tools with which to think. But that's the point. You have to think, not just know, and that makes it different from the way much medical education has traditionally been taught. Even EBM practitioners get caught because they fall back into the old ways while teaching the new, or constructing new knowledge, like systematic reviews. That's why many of them are so bad and misleading.

Every healthcare organisation should think seriously about bulk purchase and distribution to its staff. Every healthcare professional who hasn't got a copy to hand should think carefully about their future. One way or another, get it.

Evidence based resource in anaesthesia and analgesia. M Tramèr. BMJ Books (www.bmjbooks.com) pp 220 ISBN 0-7279-1437-5.

The editor has managed to bring together contributions from an interesting group of people involved in preparing and thinking about systematic reviews. In a room together, one suspects there would be some free and frank exchanges about the nature of evidence, how to get it, how to appraise it, how to use it, and whether it matters in any case. Therein is its charm.

It is not a cookbook, nor is it a paean of praise for EBM. But it is a formidable collection of essays of interest to audiences wider than just anaesthesia. The first part looks at EBM and systematic reviews in a wider context. Neville Goodman's opening chapter is all about caution, and rehearses many of the arguments about EBM. If it does nothing else, it makes you think. Egger & Davey Smith do much the same by putting the potential and limitations of meta-analysis under their "meta-scope".

The second part is more about what evidence there is and what to do with it. Pain, nausea and vomiting and more specific topics are examined in detail. The chapter on allogenic blood transfusion borders on the exciting. Cleverly, the whole gamut of systematic reviews relevant to anaesthetists is brought together at the end.

Yes, this is an important resource. Bringing evidence and philosophy together for one discipline makes sense, and works. More please.

JA Senneff. Numb toes and aching soles: coping with peripheral neuropathy. MedPres, San Antonio, Texas. 300 pp. ISBN 0-9671107-2-6 (hardback); ISBN 0-9671107-1-8 (paperback).

John Senneff is a retired lawyer who has peripheral neuropathy. His story, recounted in the preface, is that of difficulty with diagnosis and more difficulty in finding a treatment that helped. The side comments about acupuncture at $75 a pop will ring bells for those used to examining evidence.

But an evidence-based book this isn't. It's written by a patient, for patients, and with a particularly north American bent. European readers will find some of the drug names unfamiliar. Those who want evidence with a capital E will find it hard going at first.

But persevere. The book has enormous scope and is up to date (COX-2 inhibitors are discussed, for instance). Most of the medical treatments likely to be effective are there, together with some interesting and pertinent comments on adverse effects and from patients who use some of many treatments mentioned in the book. There are interesting thoughts from the patient perspective: "clinical trials, or the lack thereof, do not mean a lot when your feet are hurting and you are getting relief from magnet shoe inserts – or you think you are".

The paperback costs about $20 (by phone 1-888 633-9898 in the US). It is probably worth that for the chapter on coping alone. Practical tips from others who suffer brought together. No tirade, lots of caution about treatments, conventional and others. It wouldn't do researchers any harm to spend a few minutes with the book either.

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